From: **Patrick Frank** pfrank830@earthlink.net
Subject: Re: FOR-17-0244
Date: April 18, 2018 at 8:45 PM
To: Young, Peter p.young@lancaster.ac.uk
Cc: Derek Bunn dbunn@london.edu

PF

Dear Peter,

I offered to rewrite the manuscript. I also agreed to respond to the reviewer. This is standard practice. The reviewer identified no fatal errors of analysis.

Your rejection is therefore not justified by standard journalistic practice.

Manuscript Figures 2, 3, 4 and 9, as well as SI Figures S1, S3, S4, S5, S6, S7, S8 and S11 all demonstrate the linearity of GCM temperature projections far beyond any possible rational denial; no matter GCM inclusion of differential equations.

Your dismissal of linearity is in the face of an overwhelming and visibly obvious demonstration to the contrary. Your view is clearly incorrect, Peter, not mine.

Your paper is indeed hard-going. If my manuscript is hard-going then that just makes it similar to yours.

I know about dynamical systems. Our difference is not semantic. Our difference is that I believe what is demonstrated right before my eyes while you apparently do not.

Look again at the Figures listed above. Every single GCM projection is a linear extrapolation. Every one of them. Where, then, the evidence of non-linear dynamics?

I regret your upset, but truly your rejection of what is demonstrated remains beyond comprehension. And your journalistic process with regard to me is not ill described as specious.
You have rejected without objective cause.

Please change your web “reject” to ‘declined.’ It is the only honest description of what you have done.

Yours,

Pat

Patrick Frank, Ph.D.
Palo Alto, CA 94301
email: pfrank830@earthlink.net
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These things are, we conjecture, like the truth;
But as for certain truth, no one has known it.

Xenophanes, 570-500 BCE
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> On Apr 18, 2018, at 3:22 AM, Young, Peter <p.young@lancaster.ac.uk> wrote:
>
> Dear Patrick,
>
> Well, sadly, I do not think I can help anymore. The decision is up to you on the conditions stated in my letter and it is you who will have to make this decision. As I said, I have been in a similar situation before and I sympathise with your predicament. But the process of journal publication has rules which I, as an editor, have to follow and, while the refereeing system is far from perfect, it is the only one we have. Consequently, being as fair as I possibly can and making sure that the referees are also fair (or if not change them) is all that I can do. I feel, in your case, that you have been treated fairly but, because you have made no attempt, as far as I can see, to write your paper in a way that would be easily understood by the readership of the JOF, it is clearly unacceptable in its present form.
>
> You raise some technical points with which, I must say, I disagree, given my knowledge of global climate models and their emulators. Despite what you say, unless I am misunderstanding it, the large models are clearly in the form of partial differential equations or their discrete-time equivalents but my detailed knowledge of them is not particularly good because I have never worked with them. On the other hand, I have worked with the MAGICC emulation model and this is clearly an 80th order ordinary differential equation that I am able to emulate well with lower order models: almost perfectly with an 8th order differential equation and very well indeed with a 2nd order differential equation. Moreover, if you perturb these models with a radiative forcing input (see figure 1 in my paper) then they exhibit a clear dynamic response. We must conclude, therefore that as differential equations are models of dynamic behaviour and the models respond in a dynamic manner, they are, indeed, dynamic models. So your statement "It is therefore the climate models that are static with respect to projected temperature” is, unless you can offer an alternative explanation, not correct.
>
> Finally, can I say that I have read your paper and I found it hard going. I feel that it might help, therefore, if read my paper also; it will

Finally, can I say that I have read your paper and I found it hard going. I feel that it might help, therefore, if read my paper also. It will be hard going for you too, I expect, but I hope it will help you to understand what I (and most other scientists and engineers) mean by a 'dynamic system'. It seems to me that our different interpretations of the climate models in this regard may arise for differences in semantic understanding and, if so, this might be revealed by your reading of my paper. I hope so anyway.

I am truly sorry I cannot help any further but best wishes and best of luck,

Peter

Prof. Emeritus Peter Young,
Systems and Control Group,
Lancaster Environment Centre,
Lancaster University, UK.

My recent book Recursive Estimation and Time Series Analysis: see http://www.springer.com/engineering/control/book/978-3-642-21980-1

Our recent book True Digital control: see
http://eu.wiley.com/WileyCDA/WileyTitle/productCd-1118521218.html

For information on the CAPTAIN Toolbox and latest paper downloads:
http://captaintoolbox.co.uk/Captain_Toolbox.html
e-mail: p.young@lancaster.ac.uk
Phone: +44 1524 33558

Lancaster Environment Centre Web Page:
http://www.lancaster.ac.uk/lec/about-us/people/staff-list/all/peter-young/

> On 18 Apr 2018, at 05:50, Patrick Frank <pfrank830@earthlink.net> wrote:
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> Dear Peter,
>
> I presently have a manuscript under review at the Journal of Chemical Physics. The reviews raised points to be addressed. The JCP editor conditioned acceptance on their resolution.
>
> I presently have a manuscript under review at the Journal of Forecasting. The review raised points to be addressed. The JF editor rejected the manuscript.
>
> My CV includes 70 peer reviewed papers. The JCP response is standard. The JF response is not.
>
> This is a well-trodden path for me. It has always ended the same way.
>
> A new submission will have new reviewers who will raise new points. The logic of rejection is repeated.
>
> If you will condition publication on resolution of the present review and an acceptable re-write, I will undertake it. However, I am unwilling to participate in a losing game.
>
> With respect to your comments, the coefficients of the linear emulator are indeed derived from fits to individual model data sets.
>
> Manuscript page 9, last paragraph, directs to the Supporting Information where the method is presented in detail. That explanation can be moved into the manuscript
>
> You wrote it worries you that the emulation equation is a static relationship, while the climate is dynamic, quasi-periodic, etc. However, stated repeatedly in the manuscript is that the emulator is not concerned with the climate, or with climate physics. It is meant to reveal the behavior of climate models, and only that.
>
> The emulation equation does that extremely well. It is therefore the climate models that are static with respect to projected temperature. It should thus instead worry you that the climate models, which the equation emulates so well, apparently project temperature as a static relationship with GHG forcing.
>
> That is the point of the analysis after all: *climate models* merely linearly extrapolate greenhouse gas forcing. That's all they do. This is what you should worry about.
>
> The worry that the model emulator does not reflect the climate itself is not relevant to the meaning and use of the equation.
>
> Finally, you may be interested to know that the global temperature anomaly record is nearly meaningless. The field completely ignores systematic measurement error, which is not Gaussian and does not average away. Nor does systematic error arising from uncontrolled environmental variables subtract away. An estimated lower limit of residual measurement uncertainty is (+/-)0.5 C.
>
> If you like, I have published on this problem. The paper is open access here:
> http://meteo.lcd.lu/globalwarming/Frank/uncertainty_in%20global_average_temperature_2010.pdf (869.8 KB)

Even more incredibly, every group including BEST ignores the limited resolution of the historical thermometers.

The field neglects all this in favor of self-serving assumptions that all measurement error is random and, implicitly, that all temperature sensors have infinite resolution.

In any case, please let me know if you will accept to continue the first JF submission.

If you decide not, then please have the grace to change the web-decision to 'decline' rather than "reject," because the decision to exclude will have been made in the absence of any justifiable analytical cause.

Thank-you for your consideration, and best wishes,

Pat

Patrick Frank
Palo Alto, CA 94301
email: pfrank830@earthlink.net
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These things are, we conjecture, like the truth;
But as for certain truth, no one has known it.

Xenophanes, 570-500 BCE
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On Apr 16, 2018, at 5:03 AM, Young, Peter <p.young@lancaster.ac.uk> wrote:

Dear Patrick,

First, can I say that your statement "Honestly, your response is beyond comprehension", is not
the kind of language that encourages an editor to look again at a paper that has been rejected. In future, you might think a little more about about the way you communicate with those who are trying to do their unpaid job in as fair a manner as possible.

However, I can see you are frustrated by your experiences trying to publish material in the standard climate literature, so I will ignore this indiscretion on your part. I should stress that I have no connections or particularly good feelings towards the climate community because I too have had papers rejected by them on what I considered spurious and questionable grounds. So you should not think that any comments I make, or indeed those made by the referee in this case, are anything but our own feeling about your paper based on our own knowledge and experience. I can assure you that we are both fair in our judgements and nothing would give us more pleasure that to be able to accept a paper of yours, provided it is suitable for the JOF and is reasonably comprehensible to its readership.

On the above basis, I have now had time to look again at your paper, as well as the quite comprehensive review of the paper that was prepared by a referee. I should say that I originally looked at your paper and felt that, in style and content, it was not really suitable for JOF. This is usually the first stage in the reviewing procedure and, if I have such reservations, I usually send it to a single referee without, in any way, passing on my reservations to the referee. Indeed, I selected this referee because I know he is very fair in his judgement and is one of the few people in the forecasting community who knows about climate data and climate models. On this basis, I feel that I could do no better at reviewing the paper than this referee. His review is quite comprehensive and his critique of the paper seems most appropriate to me. If we put the technical criticisms on one side (although I think these are important and need to be answered), then the main problem that he sees with the paper is that you have made little, if any, effort to communicate satisfactorily with a forecasting audience.

I cannot but agree with the referee in this last regard; indeed it seems to me that you may well have submitted the same paper to JOF that you had rejected by a climate science paper. I hope that, with aftersight and if this is the case, you will see this was a major mistake. If you want to have a paper published in a journal then you must take the trouble to look at the kind of papers that have appeared in that journal in the past, as well as the style and detailed content of such papers. This is not an easy task, as I know from my own past experience writing papers for journals ranging from ecology, through engineering and the environment, to macroeconomics. On the contrary, it is very hard work, requiring much time and energy. But it is an essential task if you want your paper to be published.

In order to illustrate this latter point, I attach a paper of mine on global surface temperature modelling and forecasting that has appeared very recently on the International Journal of Forecasting (IJF). This was a paper I submitted to a climate journal and that was rejected by them on what I believe were entirely spurious grounds. In order to make it suitable for the IJF, I had to modify the paper quite a lot and it took me a long time. For instance, you will see that, because I am using a 'hybrid' continuous-time modelling and forecasting methodology that is not that well known to the forecasting community, I have gone to considerable pains not only to explain this methodology and even compare the results I obtain with those obtained using a more standard discrete-time forecasting approach used by most readers of JOF, but also to explain my models in the context of the traditional climate models (with which most IJF readers are unfamiliar). So you need to do the same kind of thing if you wish a paper, based on your work, to be acceptable to the JOF.

My considered conclusion, therefore, it that, if you wish to submit a new version of your paper to JOF, as a new submission, then I will be pleased to process it. However, it would need to be revised very substantially so that it is in a form that responds fully to the above criticisms. In particular, the paper needs to be shortened by removing some of the technical detail that will be incomprehensible to almost all of the forecasting audience and which, I believe, is not essential to the paper. It should have a style and content that is reasonably matched to the forecasting community and the readership of JOF, taking into account past publications in the journal. And, of course, it should respond fully to the technical comments of the referee because these would surely arise again if they are not responded to. This will, I am afraid, mean a lot of hard and time-consuming work on your part. Consequently, I need to stress that there can be no guarantee that the revised paper would be considered acceptable for publication because it would have to go through the same reviewing procedure as that used for all newly submitted papers, with at least two referees.

Finally, I should say that, on my reading of the paper, there was one thing, in addition to the points raised by the referee that worried me. Having analysed the globally averaged climate data myself, there is no doubt in my mind that these are data from a stochastic, dynamic system that appears to be characterised by short and long time constants, as well as long-term quasi-cycles. In this regard, it worries me that your 'emulation' model is a static relationship and, as the referee states, "the parameters are not estimated by fitting to a data series, they are derived from first principles": in other words, the depend on your hypotheses about the nature of the system and how it can be represented. I feel that, in any new submission, you would need to address these important issues.

Best wishes,

Peter

Prof. Emeritus Peter Young,
Systems and Control Group,
Lancaster Environment Centre,
Lancaster University, UK.

My recent book Recursive Estimation and Time Series Analysis: see http://www.springer.com/engineering/control/book/978-3-642-21980-1

Our recent book True Digital control: see
http://eu.wiley.com/WileyCDA/WileyTitle/productCd-1118521218.html

For information on the CAPTAIN Toolbox and latest paper downloads:
http://captaintoolbox.co.uk/Captain_Toolbox.html
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Lancaster Environment Centre Web Page:
http://www.lancaster.ac.uk/lec/about-us/people/staff-list/all/peter-young/

> On 14 Apr 2018, at 03:59, Patrick Frank <pfrank830@earthlink.net> wrote:
>
> Dear Prof. Young,
>
> From your extended silence, I gather you are disinclined to further consider FOR-17-0244.
>
> Is that correct?
>
> Please let me know.
>
> Thanks for your consideration,
>
> Pat
>
> Patrick Frank, Ph.D.
> Palo Alto, CA 94301
> email: pfrank830@earthlink.net
> ++++++++++++++++++++++++++++++++++++
> These things are, we conjecture, like the truth;
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<PCY_IJF_2017.pdf>